July 10, 1970

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Dear Mike:

Enclosed is the manuscript for the proceedings of the Royal Society. It really got done a bit faster than I thought it would and I hope it doesn't set back publication of the proceedings for too long. It pretty much follows the talk I gave and doesn't purport to be an original work; rather, it is a review of a particular problem with the work on the Tsa 3T3 added on. I hope it fits the bill.

I am still trying to decompress from the marvelous experience in London. I can't tell you how much I really enjoyed the visit and being able to see first hand what was going on in your lab. I talked to some of the people here about some of the implications of the DNA induction and I think that you will find Bill Folk receptive to considering the possibility of working with you on this problem; but, as you suggested, it is best to leave it until he gets there and the two of you and Lionel can work it out. As soon as I have a clear idea of what we want to do along those lines, I will write to you. Jim Peacock, who is an expert in chromosomal replication, and I were going to discuss some approaches to the problem of characterizing the type of DNA synthesis induced by virus infection as compared to serum stimulation. I'll also see Phil Hanawalt to discuss approaches for measuring repair synthesis if it occurs and how to distinguish from true de novo synthesis.

One question which occurred to me and which I can't recall seeing comments in your published work is whether abortively-transformed cells are transformable by a second infection? Is the frequency within the normal limits? Dale Kaiser has been studying a case in which certain mutations in λ phage turn off the production of repressor and thereby make the lysogen sensitive to infection. Moreover, this mutation is dominant to the wild-type allele, i.e., heterozygotes carrying the mutant gene and a wild-type gene are still sensitive to superinfection (good repressor synthesis is being turned off by the mutant). He wondered whether abortively transformed

cells might now not be transformable for some reason but I wondered whether abortively transformed cells might be more easily transformed than normal cells. Has this ever been tested?

I had meant to ask you when I was in London about this year's Lepetit Symposium. Jim Watson told me it was going to be on tumor viruses and that you were one of the organizers. Jim said that it would be some time in early November in Paris. Is that set and do you know any more about the program? I attended last year's meeting in Florence on transcription and that turned out to be a fairly interesting and useful get together. Perhaps, this year's would even be better. The reason I wanted to know is whether I should tentatively leave open the first part of November for possible attendance at the meeting.

I hope all the clones that we isolated are in good shape and resting comfortably in liquid nitrogen. We are tooling up to improve the hybridization procedure to the point where we can apply it to the cells and as soon as that's underway, I will write for the cells. My student and I are repeating the experiment with 3T3 and SV40 next week and I hope we will quickly find a way to induce DNA synthesis in these cells.

Please give my best to everyone in the lab, particularly to Joyce and say again my thanks for making the visit such an enjoyable one.

With best regards,

Sincerely,

Paul Berg

PB/i

enclosures